weakened but not destroyed vitality. At this stage the beautiful red and yellow tints are developed, which produce such a fine effect in certain kinds of scenery. Then comes more complete death, when the affinity of oxygen acts without any opposition, and the various brown tints of later autumn make their appearance, due to changes which we can imitate in our experiments with dead compounds. This may not be a pleasing way of viewing an otherwise charming subject, but I think we must all admit that it is substantially true.

H. C. SORBY

HUMAN ANATOMY AND PHYSIOLOGY

The Physiological Anatomy and Physiology of Man. By Robert B. Todd, William Bowman, and Lionel S. Beale. A new edition by the last-named author. Part 2 of Vol. i. (Longmans and Co., 1871.)

THIS part corresponds to the third, fourth, and fifth chapters of the last edition; it is now divided into four chapters, one of which is devoted to a general consideration of the properties of tissue, and the others contain detailed accounts of the connective, cartilaginous, osseous, and adipose varieties. Dr. Beale seems to have spared no time or trouble upon the present part, which has been carefully revised throughout; a considerable amount of new matter has been added, and many parts, especially those relating to the development of the different tissues, have been entirely re-written.

The chapter on the forms of connective tissue is very full and complete, and compares very favourably with that in the last edition; descriptions of several well-marked varieties, which were before omitted, being now introduced, such as those occurring in the Whartonian jelly, the vitreous humour, and the cornea. With respect to yellow elastic tissue, Dr. Beale states that the fibres, usually considered to belong to it, which are found in tendons, and resist the action of acetic acid, are not of elastic nature at all, but are merely imperfectly-formed white fibrous tissue; and in his account of areolar tissue he strongly contests one of the most generally-received pathological doctrines of the day, that which supposes in many cases of degeneration that the interstitial areolar tissue of the organ is the active agent, becoming hypertrophied, and then contracting and compressing the structures in its meshes. Dr. Beale considers, on the contrary, that in most cases the areolar tissue is quite passive, and that the phenomena ascribed to it are really produced by the rapid multiplication of parts of white blood corpuscles which have passed through the walls of the blood vessels.

In his account of cartilage Dr. Beale dissents from the opinion held by some, that the capsule of a cartilage cell differs from the matrix in its origin and nature; he points out that in some cases there is no matrix, in others no cell-wall can be demonstrated as distinct from the matrix; and again, in others the capsule passes gradually into the matrix; and maintains that the matrix when present is entirely formed of old capsules, and is *not* developed independently of the cells. Fibro-cartilage and elastic cartilage are both well described; no mention at all of the latter form was made in the previous editions.

The chapter on bone contains a good account of its histological structure, but is chiefly interesting from the views put forward as to the mode of origin of the canaliculi. Virchow states that they are formed by the deposition of calcareous matter round processes radiating from corpuscles contained in the lacuna, while Kölliker thinks they are formed by resorption after the lacuna has been entirely surrounded by calcareous matter. Dr. Beale differs from both—he says the bone corpuscles of the lacuna have frequently no processes, and that when processes are present they are always much shorter and much less numerous than the canaliculi, and he points out that the formation of these little channels begins at their distal end, not at the end next the lacuna, as has been supposed; his own view is that in an early stage of the development of bone, it is all permeable to nutrient fluids, but that as calcareous matter is deposited this permeability gets restricted to constantly narrowing channels, which ultimately remain as the canaliculi, and are at first filled with soft matter (cartilage or membrane), which in fully formed bone dries and shrivels up, leaving the canaliculi as true tubes.

The concluding chapter, that on adipose tissue, is on the whole good, but in the account of its histological structure the impression is conveyed that an adult fat cell consists merely of an envelope containing oily matter—no mention being made of the fact that by proper treatment a nucleus also can be almost always demonstrated. Dr. Beale considers that the fatty matter contained in the cell is formed by the degeneration of the mass of "bioplasm," or "germinal matter," of which it was once entirely composed.

The part is illustrated by a large number of very good figures, and several full-page plates.

OUR BOOK SHELF

Elementary Treatise on Natural Philosophy. By A. Privat Deschanel. Translated and edited by Prof. Everett, M.A., D.C.L., &c., Professor of Natural Philosophy, Queen's College, Belfast. In Four Parts. Part 2.—Heat. (London: Blackie and Son.)

THIS work is intended to be an elementary treatise on the science of Heat. The remarkably fine engravings that embellish it throughout, give it an air of reality which, unfortunately, is not generally possessed by English scientific books. Still, some of the original engravings might have been improved; for example, figs. 223, 240, 245, and 264 are peculiar, and do not represent what is likely to be seen in the laboratory. Having said this much in favour of Prof. Everett's translation, we cannot avoid making some unfavourable criticisms. We decidedly object to the numerous formulæ and equations which may almost be said to disfigure many of the pages; they are not sufficiently explained for a popular work, and might have been more compressed if intended for advanced scientific students. And seeing that formulæ and explanations usually vary inversely as each other within the same volume, we should have been pleased—indeed, we expected—to find as many of the former eliminated as possible. This expectation was occasioned by the translator himself, who complained that oftentimes we are confronted with "unexplained formulæ, which burden the memory without cultivating the understanding." Can Prof. Everett assert that he has explained the formula on page 362 of Part 2? Has he not rather fallen into the very error which he so ably deplores in his preface; failing to see amid V, KT, and other algebraic mystifications, that his H and h are

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not directly comparable: that the mercury in Gay-Lussac's tube is hot, while a barometer is generally cool? A student of Nature will scarcely be taught much that is satisfactory concerning Gay-Lussac's beautiful method of determining vapour densities, by being led away at once into intricate formulæ "which burden the memory, without cultivating the understanding." This one example will sufficiently indicate the fault which runs through the whole volume before us. Much new and valuable matter, albeit besprinkled with formulæ, has been added by the translator; and various passages in the original have been modified or otherwise corrected. But, though we have no hesitation in saying that the original has been thereby improved, yet the final result is neither remarkable for its novelty, nor edifying from its simplicity.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Thickness of the Earth's Crust

I TAKE in NATURE in parts. Your part for May last has just reached me in Calcutta, and is somewhat rich on the question of the thickness of the earth's crust; for three of its four numbers contain letters on that subject. First, my letter to you, p. 28; two critiques upon it from "A. J. M." and Mr. Green, p. 45; and a third from Mr. David Forbes, p. 65.

Reply to "A. J. M." I do not think it safe to draw inferences from a comparison the members of which differ so materially; a plate of lamp oil, probably a quarter of an inch deep, and the solid crust of the earth resting on a fluid mass 3,856 miles deep, supposing the crust, according to the thin-crust theory, to be 100 miles thick. To be sure, the motion in the experiment is very much greater than precession; but the depth of fluid is also very different, and the cases are not parallel. Moreover, in the one case, the oil rests on the plate, weighed down by gravity, and is easily carried round bodily with the plate. This is different to the hard crust rubbing over a depth of fluid. I see the writer, like M. Delaunay, relies much on the extreme slowness of the motion. More on this soon. We shall have to beware how we adopt the phrase "a rope of sand," for if we use the rope *slowly* enough, it may become in our hands a "rod of iron."

Reply to Mr. Green. By "pushes" I did not mean mechanical knocks, but merely geometrical movements. The pole of the earth goes round the pole of the ecliptic in a circle in about 26,000 years. I divided this circle into a multitude of little pieces. Nor was my description meant to represent this motion as being by fits and starts, but only in portions, to assist the mind in a popular explanation, not of precession, but of the thing I wanted to be understood, the slipping of the hard crust over the fluid. I think it is very likely my own fault that my description has been misunderstood.

Reply to Mr. David Forbes. Mr. David Forbes glides out of the discussion on the plea, that after all I make out that Mr. Hopkins' calculations were based only on "an idea." But so is M. Delaunay's opinion based on an idea. We must start with an idea. Ideas are of two kinds, sound and unsound; and if the idea we rest upon is sound, we cannot possibly do better than build upon it. M. Delaunay thinks his idea is supported by an experiment. I have tried to find a description of the experiment, but without success. I altogether doubt whether any experiment with models can be devised to lead to trustworthy results, regarding such a huge mass as the earth affected by such slight motions as precession. Mr. David Forbes says he went to the Royal Society to hear a paper of mine read on the constitution of the solid crust of the earth. It was natural that he should suppose from the title, that it might bear upon this question of the thickness of the crust. But this is not the case at all, as he would find. M. Delaunay's strictures on Mr. Hopkins' investigation seemed to me so important, that I did *drag in*, I may say, an allusion to them in a note. It is very likely that, as my paper was almost entirely a calculation of mathematical formulæ and their numerical application to the pendulum observations lately made in India, the paper itself was not read aloud, and that conversation turned on the incidental note.

I propose now to view the subject in a new light, and to begin *de novo*; and I feel confident that your readers will see that there is more to be said for Mr. Hopkins' method than they have by this time been led to think.

The precessional motion is no doubt extremely slow: and that because the precessional force is extremely small. But the particles of the earth's mass have a good deal more to do in the matter than to partake of this small motion. Every twenty-four hours they have to undergo a strain, first this way and then that, such that I believe no fluid, however viscous, could sustain. Though the precessional force is so minute, it is the resultant or residuum of an almost infinite number of other disturbing forces nearly balanced; as I proceed to show.

Let G be any point in the earth's mass; b its distance from the centre; a the radius of the earth; c the distance of the sun; θ the angle between b and c; S the sun's mass. Then, considering the sun's action by itself, its attraction on G I resolve parallel and at right angles to c, and from the former position subtract the sun's attraction on the *disturbing* forces on G which are wanted. These are, neglecting the smallest quantities,

$$\frac{4Sb}{c^3}\cos\theta \text{ and } \frac{Sb}{c^3}\sin\theta.$$

For the sake of a name I will call the plane through the earth's centre, at right angles to the line joining it and the sun, the Boundary Plane, as it intersects the surface almost exactly in the boundary line between sunlight and darkness. It will be observed that at this plane the first of these disturbing forces equals zero, and is positive on the sun side of the plane, and negative on the opposite side. The amount of this force is the same at all points lying in any plane parallel to the boundary plane, and the aggregate of the positive forces on the one side, and of the negative on the other, would be in each case a force acting through the earth's and sun's centres, but for the slight deviation of the earth's figure from a sphere, and the consequent arrangement of its mass. Suppose, for argument's sake, that the particles of the earth's mass are held invariably together as a rigid body; then the disturbing forces parallel to c will amount to an aggregate positive force, and an aggregate negative force, tending to separate the two parts of the earth formed by the boundary plane. These forces are equal to each other, and pass nearly through the earth's centre, at opposite and equal distances from it. They form, in fact, a mechanical "couple," and twist the earth round some diameter lying in the boundary plane. The arm of this couple is a minute quantity depending upon the ellipticity of the earth's figure. Hence the movement of the couple, from which precession and nutation arises, is a minute quantity, while the *force* of the couple (which is the tension of the earth's mass perpendicular to the boundary plane) is not of that minuteness. The other disturbing forces, represented by the second formula, all tend towards c, and compress the mass. They would all balance each other, were the earth spherical. The resultant of these forces on the sun side will be a minute quantity of the order of the ellipticity, and will act at a certain distance from the centre; and the resultant on the opposite side will be an equal force, acting at an equal distance from the centre, but in an opposite direction; so that again there will be a couple of minute power, assisting with the other couple to produce the combined motion of precession and nutation. In this case, although the forces nearly balance each other on each side the boundary plane, parallel to it, and but a small resultant follows, yet the particles of the mass have to undergo the compression from opposite sides, as in the other case they have to sustain the tension caused by the opposing forces tending to separate the two halves of the earth.

The moon will produce forces precisely similar to these, and a little more than twice their amount. The forces of the sun and moon do not come to their maxima and minima at the same time, except at new and full moon. At other times they partly conspire or counteract each other according to their position. Sun and moon together will produce most irregular cross-strains through the mass, if the particles are held together by any degree of rigidity.

It will be seen, then, that as within twenty-four hours *every* particle of the mass in its rotation is carried through the boundary plane *some* days in the year, and most of the particles every day, every part of the mass will be periodically subject to the maximum strain I have described as taking place at that strain, and most parts twice a day, as well as the compressing force at right angles to c.

The same will be the case, and at different times, except at new

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